

Dear Editor,

we would like to thank you and both referees for your evaluation and constructive comments to improve our manuscript. We have modified the paper trying to take into due account all comments and suggestions.

This resulted in many changes with respect to the original paper.  
A point-by-point answer to the referees' comments is provided here below.

Best regards,

Rome, 2<sup>nd</sup> April 2014

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Alberto Armigliato  
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## RESPONSE TO REFEREE #1

### GENERAL COMMENTS

The present paper discusses tsunami hazard quantification over a section of the Pacific coastline covering southern Central and northern South America. Only local earthquake sources are considered (i.e. non-seismic and far field source are omitted), and the hazard evaluation is regional. The tsunami hazard in the region is previously little studied, and the paper demonstrates it is significant. The study is therefore timely. The applied methods seems reasonable for a first order approach for providing mean estimates.

However, I feel that the paper includes a number of points that needs major improvement before the paper could be accepted for publication. The most important improvement points to be made are in part linked to discussion and benchmarking of the hazard methodology (1), and in part linked to the clarity of the presentation (2). Below, I provide some elaboration to points 1 and 2, these are partly repeated also under the specific comments. In addition, there are some smaller improvement points listed below, including corrections.

1 - The authors utilize databases extensively, including tsunami and earthquake databases as background for statistics, i.e. Magnitude - Frequency distributions. Key results are hazard maps for 50, 100, and 500 year return periods. At the shortest return periods, run-up up exceeding 4-5 m are observed. However, these results are not compared against run-up distributions from publicly available databases that span several hundred years (although the more recent events are covered better). This opportunity of comparing the results against field data, at least for the 50 year return period, is not utilized. Secondly, the results for the smallest return periods seems somewhat high.

The comparison has been introduced by taking into account the NOAA/NGDC tsunami run-up database. The details are provided in the text.

A worst case approach is taken by assuming shallow rupture, but this approach may be biased for the smaller magnitude earthquakes. I strongly suggest that the authors provide a more in depth discussion on the applicability and strengths / weaknesses of the model using the present case. As the applied method appears to be a hybrid of more conventional methods such as worst case scenarios and PTHA, a discussion of how the applied method related to these is recommended. A short paragraph was found in the conclusion section, but this is not sufficient. Finally, citations to the work of other groups may be given somewhat more emphasis as this is presently a bit one sided.

We have added a paragraph in section 1 where we give proper credit to different studies in PTHA, and suggest the main difference between our version and other groups' version of the PTHA.

2 - Although parts of the paper appears well written, the presentation needs clear improvement. This is due to an unclear presentation and structure of the paper rather than due grammatical errors. Several explanations are spread into different parts of the paper. Moreover, essential input such as source parameters must be explicitly given. I would recommend the authors to review the paper to provide a more clear presentation, and perhaps reduce the length. Avoid the use of repetitions and ambiguous statements. I have provided some examples below.

We tried to restructure as much as possible the paper to improve clarity and avoid repetitions. Parameters are provided where relevant. Further details are given in response to specific comments.

### SPECIFIC COMMENTS

Abstract line 15: First you discuss "a statistical" approach. Next, you say that "A deterministic approach is then used". Please reformulate, i.e. clarify that the statistical input is used as input to the deterministic approach that are conducted for different return periods.

The sentences were re-formulated in order to make this point clearer.

Page 2984, line 23: Please review if this (area and population) is relevant information, as you are investigating a coastline section. In general, the paper would benefit from cutting some superfluous information to reduce its length.

We removed the entire paragraph.

Page 2985, line 7: It seems possible to me that a landslide could have triggered this tsunami as its magnitude is low given the strength of the tsunami? The authors may wish to make a point out of this.

We have reworked the entire chapter, so that we only cite the occurrence of the 2012 event and we provide no further information or possible interpretation.

Page 2985, lines 15-20: The statements made here are partly conflicting those above, i.e. that tsunamis are partly forgotten hazard in this area. Please clarify.

We removed the sentence concerning the increased tsunami awareness by the authorities. In this way no further conflict should exist.

Page 2985, line 22: Revise wording of sentence starting with "The need of setting..."

We removed the entire sentence.

Page 2985, line 27: You should state your main assumptions here as well, i.e., that you solely look at local earthquake sources.

We added a sentence introducing this assumption.

Page 2986, line 1-6: The description of the hazard methodology is confusing (see above comment for abstract). The introduction is generally lacking a discussing of other hazard methodologies. Examples of other methods include for instance worst case scenario based methods (e.g. Lorito et al., 2008), or PTHA (e.g. Geist and Parsons, 2006; Power et al., 2007; Gonzalez et al., 2009)

Page 2986, line 9: Please provide a reference to the Catalogue.

We added a reference to the Catalogue by Molina (1997).

Page 2986, line 13: I would be careful with using the term "GR law", rather use "GR relation".

Re-phrased.

Page 2988, Section 2.1: This section also review past work on tsunamigenesis, consider renaming.

The section became section "3" and is now called "Observed tsunamis".

Page 2988, line 18: Please elaborate on why the Pacific is more tsunamigenic.

The reworking of the entire section led to the deletion of the relevant sentence, so no further discussion is conducted.

Page 2989, line 23: Please provide reference to the Imamura-Iida scale.

Two references added.

Page 2990, line 1-2: Is it realistic that all past tsunamis are caused by earthquakes? Experience from other regions with limited earthquake potential may indicate that landslide sources are underreported (e.g. Løvholt et al., 2012a).

The focus here is only on the Caribbean coasts of Central America. Although it is impossible to exclude the occurrence of historical landslides as possible sources of (unreported?) tsunamis, nonetheless the catalogues by both Fernandez et al. (2000) and Harbitz et al. (2012) agree on earthquakes being the only known cause of historical tsunamis along the Central America Caribbean coasts.

Page 2994, lines 3-10: Different GR relations have been used by others. Some reasoning for the suggestion of the present GR-relation is suggested, see e.g. Geist et al.(2009).

The subject introduced by the Referee is undoubtedly interesting, but in our view a reasoning on it is out of the scope of our paper. Nonetheless, we included a sentence pointing the reader to the papers by Kagan (2002) and by Geist et al. (2009).

Page 2994, line 20: Sentence starting with "The main idea ...". Please revise wording or remove sentence.

The sentence was removed.

Page 2994, line 21: Sentence starting with "The occurrence ..." and next sentence does not make sense to me. Please revise wording to clarify. Here, the authors repeat above statements which is confusing to the reader.

Although we did not find the sentence so confusing, we followed anyway the referee's suggestion and modified the sentence entirely. Hopefully the message is now clearer.

Page 2995, line 1: If the fault parameters are deduced from the magnitude the reference should be given. But such references are stated later in the text. The paper would highly benefit from a more compact presentation of the method.

References added also at this stage.

Page 2995, line 10-11: Here, the authors should preferably include reference to other groups in addition to their own.

Some references added (Løvholt et al., 2006; Lorito et al., 2008; Harbitz et al., 2012; Løvholt et al., 2012).

Page 2996, line 6-7: Please explain more explicitly how the activity rate is reduced.

We have added a panel to Fig. 5, where the numbers of tsunamigenic earthquakes per magnitude class are displayed. We have also added a couple of sentences intended to clarify how the reduction works.

Page 2996, line 19: Again, the method should be explained more explicitly. For instance, long near shore sources would have practically no spreading, whereas short distant sources are attenuated. Please elaborate.

We added the reference to Comer (1980) at this stage, commenting with a sentence incorporating the Referee's suggestion.

Page 2997, line 7: The methodology follows closely many of the procedures previously published by Løvholt et al. (2012ab), and a reference must be included, either here or above.

A reference to both papers, and also to Kanoglu and Synolakis (1998) work, was added some lines after this point.

Page 2998, lines 10-13: The reader should be notified that there may be substantial corrections to this assumption.

We added a sentence summarizing the main approximations involved.

Page 2998, lines 14-19: This is partly stated above, repeated statements such as this should be avoided. Although we agree that repeated statements should be avoided whenever possible, this sentence here is definitely needed by the way we structured the presentation in this section.

Page 2999, eq 5.: A reference to page or equation number in C&G would be helpful to the reader.

We corrected the reference to Pelinovsky and Mazova (1992) and indicated the number of the equation in the original paper that we used.

Page 3000, A discussion regarding the validity may be done for instance by performing a (simple) comparison with run-up data from a database such as NGDC. As some conservative assumptions are made, there be implications on the results that needs to be discussed here.

Comment appreciated and accepted. We performed a comparison with the NOAA/NGDC database and discussed the results in the text.

Page 3000, line 25-30: These assumptions should be placed up front.

We instead think that it is more useful to comment first on the results and hence discuss on the limitations of the approach.

Page 3001, line 10: The authors assume shallow rupture, which indeed characterize tsunami earthquakes in addition to low rigidity. However, the rigidity is rather large (50 GPa). This is a strange assumption given that the earthquake scenarios are shallow, and hence this is underconservative. However, it may balance other conservative assumptions. In general, the authors should explicitly state their earthquake parameters as the method would be more transparent.

We accept the Referee comment, and changed the value to 30 GPa, justifying this by citing the paper by Geist and Bilek (2001). At the same time, we are sure the Referee will agree on the fact that the choice for the shear modulus is often based “on common practice” rather than on “experimental” or “well known” values.

Regarding the other earthquake parameters, our impression is that they are already clearly stated.

Page 3001, line 20: Do you mean in this region? Globally, there are many more examples.

Yes, but they remain a very little fraction of the global tsunami occurrences (see for instance Bilek and Lay, GRL, 2002)..

Page 3002, line 25: Note that Gonzalez et al. (2009) found that local earthquakes tend to dominate the hazard. The authors may want to make a point of this.

We accept the Referee’s comment and added a sentence citing the Gonzalez et al. (2009) paper.

Page 3003, lines 18-25: As mentioned above, PTHA should also be discussed.

Some technical corrections:

Abstract line 10 - Remove "first" and "then".

The sentence has been removed as part of a general abstract reshaping.

Page 2986 line 25 - Remove "at the moment".

Removed.

Page 2988 line 13 - Remove "little".

We are not sure whether removing “little” maintains the sense of the phrase. We want to stress that the awareness is lower than it should and could be.

Page 2995 line 26 - Replace "not so numerous" with "limited"

Corrected.

Page 2997 line 4 - Reword sentence starting with "They tend to be ...", i.e. "The profiles shown in Fig. 10 are almost equally ..."

Done.

Page 3001 line 1 - Replace "I deed" with "indeed"

Corrected.

#### References:

Geist, E., and T. Parsons (2006), Probabilistic analysis of tsunami hazards, *Nat. Hazards*, 37, 277–314

Added.

Geist, E.L., Parsons, T., ten Brink, U.S., and Lee, H.J., (2009), Tsunami Probability, in Bernard, E.N., and Robinson, A.R., eds., *The Sea*

Added.

González, F. I., et al. (2009), Probabilistic tsunami hazard assessment at Seaside, Oregon, for near- and far-field seismic sources, *J. Geophys. Res.*, 114, C11023, doi:10.1029/2008JC005132.

Added.

Lorito, S., M. M. Tiberti, R. Basili, A. Piatanesi, and G. Valensise (2008), Earthquake generated tsunamis in the Mediterranean Sea: Scenarios of potential threats to Southern Italy, *J. Geophys. Res.*, 113, B01301, doi:10.1029/2007JB004943.

Added.

Løvholt F., Kuhn D., Bungum, H., Harbitz, C.B., Glimsdal, S., (2012a). Historical tsunamis and present tsunami hazard in eastern Indonesia and the southern Philippines, *J. Geoph. Res.*, 117, B09310, doi:10.1029/2012JB009425

Added.

Løvholt, F., Glimsdal, S., Harbitz, C.B., Zamora, N., Nadim, F., Peduzzi, P., Dao, H.I., Smebye, H., (2012b). Tsunami hazard and exposure on the global scale. *Earth-Science Reviews* 0012-8252. doi:10.1016/j.earscirev.2011.10.002

Added.

Power, W., Downes, G., Stirling, M., (2007). Estimation of tsunami hazard in New Zealand due to South American Earthquakes. *Pure and Applied Geophysics* 164, 547–564.

Added.

Further references, not listed here, were added.

## RESPONSE TO REFEREE #2

The topic of the paper is suitable for the journal since it addresses an issue of interest to the scientific community. Assessment of tsunami hazard for the American Pacific coast from southern Mexico to northern Peru has been analyzed with interesting conclusions, considering the lack of studies in this region. However, it cannot be said that the applied method or the conceptual approach and results stand a significant or novel contribution even if they don't lack scientific rigor. Additionally, the method assumes many hypotheses and so, the results can only be taken as an approximation. The considerations explained in this review must be taken into account before the paper could be ready for publication.

The authors explain the assumptions they take into account. Besides, they recognize the simplicity of the methodology, and explain that it can only be taken as a preliminary analysis. But, even if these issues are commented, I miss some validation of the methodology. The authors must give a comparison between their methods and some numerical simulation results or real field data in order to give some validation to the methodology. From my point of view, even if future research aspects in this direction are commented in the conclusions chapter, some validation should be given.

Given that numerical model simulations are not used, Green's formula is applied instead of calculate the propagation of the wave. Refraction and reflection are not considered in the formula so the requested validation will measure if the influence of these processes is important or not.

In general, concepts are widely explained and repeated in the paper. The authors must detail them more concisely, what would reduce the length without losing significance and providing clarity to the article.

### SPECIFIC COMMENTS

This reviewer would like to give some comments or suggest corrections in order to increase its overall significance.

-Page 2984, Line 10: The text", a statistical in<sup>st</sup> and then a deterministic analysis", is not clear. After that, in 2985-L 28, "hybrid probabilistic-deterministic analysis" is used, what expresses better the concept of the methodology.

The sentences were re-formulated in order to make this point clearer.

-Page 2984, Line 23: Maybe the population living at coastal sites could be given (if available) instead of the total population, because, actually, they are the affected group in case of tsunami.

We decided to delete the entire first sentence of the introduction. As a result, this information was also removed.

-Page 2991 Lines 9-14: Please elaborate on the decision you made about the use of earthquake catalogs to perform the statistical analysis. What other options have you considered? And why have you rejected them?

We have reworked the sentence a bit, but feel that the main point is already there. From the statistical point of view, we had just the two options we indicated: performing the statistical analysis directly on the events listed in the tsunami catalogue or in the earthquake catalogue. Since the main requirement to obtain stable/reliable statistical results is to work with populations of events as large as possible, the choice fell on the earthquake catalogue, which includes a number of items an order of magnitude larger than the tsunami catalogue.

-Page 2993, Line 20: Please clarify what completeness periods are.

We have added a sentence specifying the general meaning of completeness period.

-Page 2996, Line 11: Please elaborate on the decision of choosing the transects associated to bathymetry normal to the trench. Intuitively, the transects normal to the coast adjust better when Green's formula is used.

We added a sentence to stress the fact that either choice is reasonable, but our choice is made with the intention of taking fully into account the effect of source directivity. Taking instead transects perpendicular to the coast may maximize the impact effect, but would generally reduce the amount of tsunami energy propagating from the source along the transect

-Page 2997, Line 3: Please explain the criteria you used to select the 130 bathymetric profiles and why you have considered that this number is enough to give a good approximation for the run up distribution along the coast.

-Page 2997, Line 4: Rewrite the sentence "They tend: : :"

The sentence has been rewritten, based also on the input of Referee 1.

-Page 2997, Line 8: You have simplified the transects by considering 2 ramps. This assumption implies some limitations that should be explained in the text of the paper, as well as other assumptions limitations are properly detailed.

We added a sentence based on this comment.

-Page 2998, Line 2: An explanation about the election of the rigidity value ( $5 \times 10^{10}$  Mpa) must be given in order to understand why it is suitable in this case.

Based also on the comment by Referee 1, we changed the value to 30 GPa. A short justification is provided in the text.

-Page 3002, Line 6: Local effects are not taken into account, as detailed on general comments. The influence or not of these processes could be measured by comparing some results to real or simulated cases, and a conclusion in this direction should be added.

Although this comment is correct per se, at the same time it would require an effort that is beyond the initial scope of the paper. We claim the possibility of restricting our tsunami hazard assessment approach to the hybrid method we described and applied, leaving to possible future studies the comparison with more detailed and refined numerical simulations.

#### OTHERS:

-Page 2987 Line 26: The sentence "The cocos-Caribbean: : ...along the junction" needs a stop.

We have put a stop dividing the long sentence in two parts.

-Page 2988, Line 19: Replace estimation with estimationS

Replaced.

-Page 3001, Line 1: "indeed" instead of "I deed" must be written.

Corrected.

Figures:



Figure 4 contains the seismic catalogue AMB\_AD\_NOAA. And the figure 5 is exactly the same but shows the zonification. Just one of the figures is necessary because the data contained on figure 4 can perfectly be referred to.

We agree on the comment and “joined” the two Figures.